United States Department of Agriculture | Forest Service | Pacific Northwest Research Station | Pacific Wildland Fire Sciences Lab
400 N 34th St. Suite 201
Seattle, WA 98103

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To whom it may concern

As requested, I have reviewed selected sections of the Draft Fisher Species Report developed by the US Fish and Wildlife Service. Please find below comments relevant to each section.

Wildfire (pp. 58-71)

Overall this section is a good overview of many aspects of wildfire as they pertain to fisher habitat and life history. As I noted when agreeing to review, I am not familiar with the Fisher literature, but the authors have obviously looked at a lot of the research and provided a fairly detailed overview of it, with reference to each of the distinct sub-regions. There is some confusion at the beginning regarding terminology, and later regarding the material in Tables 6 and 7.

p. 58, 2nd paragraph: Fire severity. I believe the standard usage now is low, high, and mixed severity. See Halofsky et al. (2011) for a good overview. The term "mixed severity" allows for patches of different severities, and subsumes your terms "moderate" and "medium". Depending on the spatial scale of analysis, it could be said that most fire in the regions of interest is of mixed severity. The distributions of patch sizes are important, as I believe you note further down, as large high-severity patches may fragment habitat, even if they are not the dominant severity.

p. 58, 3rd paragraph: The natural fire rotation, or fire cycle, is probably a better measure of frequency for much of the area you are considering. Where many or most trees are killed by fire, it is the only way to calculate fire frequency. Fire-return intervals, whether at points or composites, are almost always calculated from repeated measurements (dating of fire scars) on the same trees. This is particularly relevant for western Washington and northwestern Oregon.

p. 59, line 1: In SOME regions and forest types, but not all. Note that the Skinner et al. citation is just for the Klamath region.

p. 60, 2nd paragraph: The first part of the second sentence is almost universally false. Because of changing drivers -- climate, vegetation -- past fire severity in mixed-severity regimes is usually a misleading predictor of the future. Second half of the sentence is a complete *non-sequitur*. Next sentence: It's not the fire in shrublands that would "erode"

the forest, but the rapid return of fire that could kill young trees at their most vulnerable stage, preventing recruitment. This is the case with high-severity fire only, where there is no surviving seed source.

p. 60, 3rd paragraph: Hanson is a strong advocate of hands-off management, to the degree that I and some others believe his science can be affected. A paper a few years ago in Conservation Biology seriously misrepresented fire regimes in the Pacific Northwest. I would encourage you to find some confirmation, however indirect, in the peer-reviewed literature, for this observation.

p. 61, 2nd paragraph: You make a good point about the issue of spotted owls mid-paragraph, but it might be stronger to highlight the so-called "extinction debt" created by loss of habitat, which I believe we are now seeing (although I haven't kept up with the monitoring) in shrinking spotted owl populations even though habitat is now more or less stable.

p. 62, last paragraph: There is a scale confusion here. rather than contrasting severity types, it's really about the sizes of high-severity patches, regardless. You could check out and cite Cansler and McKenzie (2014) regarding changing high-severity patch sizes in recent fires.

p. 63, 2nd paragraph: It is often claimed that fire frequency is increasing, but that is a meaningless statement without reference to the spatial scale of observation. What we can say is that in some areas the annual area burned is increasing (but in others it is not).

p. 63-64,Table 6 and 7: I would question the usefulness of these. Two wrongs do not make a right. If you are going to present these numbers as if they are meaningful, you should (1) do the tedious but straightforward GIS exercise of overlaying the 27 years of MTBS data to quantify the double-dipping, (2) specify the percentage increase in future fire that you are assuming (and we know that our models are wrong -- see my comments on the climate-change section), (3) check out Kolden et al. (2012) for information on the proportion of unburned area within the fires, which affects the double-dipping. Much of this also applies to Table 7.

p. 66. Future projections of area burned. I wouldn't use numbers like an 8-fold increase. These come from very noisy models (I know -- I was a coauthor), from which strict extrapolations will produce overestimates. We don't really know what's going to happen in these west-side forests. The best analysis so far is Gedalof et al. (2004).

p. 67: Good to note that one large fire can cause big changes, and that spatial pattern matters. Some discussion of extreme fire events, whether here or in the climate-change section, might be appropos. A good reference is Stavros et al. (2014).

Climate change (pp. 72-85)

There are two general problems with this section. First, the references to the IPCC are out of date. We now have the Fifth Assessment Report, with new model runs using the Representative Concentration Pathways (RCPs -- van Vuuren et al. 2011) instead of the old emissions scenarios. The results are very similar, so much of what you say still holds, but I think you'll want to have the latest citations. The second issue is that you are extrapolating numbers by going way out (to 2070-2100) then interpolating. This reminds me of dialog from an old movie:

"How do I estimate the size of the herd?"
"Count the legs and divide by four."

Projections for the late 21st century are an order of magnitude less certain than those for mid-century, because of the cumulative error associated with longer runs of the models plus that associated with the many feedbacks in the Earth system. (see McKenzie et al. 2014 if you want a heavy dose of this). I find it hard to believe that studies that project out to 2100 don't have intermediate results -- at least some of them. I'm not about to review everything you cited here, but please think about how you could use mid-century projections, even if there are fewer of them.

p. 74, Temperature: May be worth noting that 2014 appears to be the hottest year yet.

p. 76, last 5 lines. Here you mention that fishers don't do well in heavy snowpack, but nowhere (unless I missed it) in you conclusions do you note that this could INCREASE potential habitat in the future.

p. 79, 3rd paragraph: Again, this 8-fold increase is a model extrapolation, which we know to be wrong. Last paragraph: you could cite the Cansler & McKenzie if you want on high-severity patch sizes. Don't use Westerling et al. (2006) with respect to future projections. These correlative models are not robust because the authors cherry-picked the time period over which to develop them, evidently for maximum effect.

p. 80, 2nd paragraph: Hicke et al. (2012) is a good overview of the interactions between fire and bark-beetle outbreaks.

p. 80, Summary: Here is where the snowpack issue should come up.

p. 81: Really bad idea, this cart-pulling-horse approach.

References

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Don McKenzie
Research Ecologist